From:	Jay Field
To:	PETERSON Jenn L
Cc:	Eric Blischke/R10/USEPA/US@EPA; pj.bridgen@eiltd.net; jeremy_buck@fws.gov; Joe Goulet/R10/USEPA/US@EPA; Burt Shephard/R10/USEPA/US@EPA; Robert.Neely@noaa.gov; OMEALY Mikell; chris.thompson@eiltd.net; Chip Humphrey/R10/USEPA/US@EPA
Subject:	Re: Summary of the November 21st Benthic Meeting
Date:	12/08/2005 10:44 AM
Attachments:	wr_chems_n_qual.xls

Jenn,
thanks for your comments (and a copy of the memo from Windward). A couple of quick comments to the memo.
As you pointed out, Windward assumed agreement about several issues that I did not think were resolved in the meeting:
1) FPModel endpoints: I do not think we agreed that the FPM should use only the individual endpoints. We certainly did not agree that the FPM should ignore the Hyalella growth endpoint. Poor model performance is not a good reason to ignore the most sensitive endpoint. As we pointed out in the July meeting, in the memo to LDW, and again in the 11/21 meeting, the pooled growth/survival results for the each species is a better way to look at the growth results because growth is not independent of survival. If they insist on using the 4 individual endpoints, I think it is reasonable to ask them to apply the FPM to the pooled results for each species as well.

2) control-normalized results: I still do not agree with Teresa's approach to control-normalization (subtracting the control result from the test result rather than dividing test by control as is commonly done). She also appears to be using this approach for growth (which I think is different than in their original benthic methods memo). Teresa is following (creating) her own precedent. However, I do agree that the results for this data set are likely to be minimal.

3) N-qualified data: attached is a spreadsheet summarizing (min, max, and number of samples) the N-qualified results for each chemical. Note that there are some high concentrations that would be excluded for a variety of chemicals. I have not been excluding them in my analyses (I apparently missed the discussion of this qualifier).

4) summing chemicals: I am using total PCBs. Because of the strong correlation between individual PAHs and LPAH and HPAH, I do not think it's a significant problem to use LPAH & HPAH instead of the individual PAHs. I have not looked at the relationship between total DDTs and the six isomers. If they use the summed values for PAHs and DDTs, they should show the basis in the report (consistent composition throughout the study area).

5) in my view, the most critical issue at this time is to resolve Mike A's issues on the FPM, to make sure that the final model results are reproducible.

Jay

PETERSON Jenn L wrote:

>I quickly looked this over, and had a few comments (enclosed with my
>comments added in). Also, there were some things brought up at the
>meeting for agreement (I think) that are not mentioned here and I am not
>sure we agreed with. Overall, here are some of the issues I see for the
>modeling effort. I briefly summed up some of the technical issues I
>have, both technical and larger issues. Anyone please jump in, dismiss
>or clarify my issues, or add to this summary!
>
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>
>Technical Issues:
>
>1. Summing contaminant classes versus using individual contaminants in
>the model: The proposal from the LWG was to sum DDTs, PAHs and PCBs (I
>think that was all). However, Mike and Jay are currently not summing.
>It seems better to sum at a later stage. For example, with the FPM Mike
>is running, you can see if contaminants are correlated and maybe should
>be summed from the results of the analysis. If contaminants are
>co-varying they will show this by where they "float" in the analysis".
>If this is shown, summing at that point makes sense, but maybe not
>before. It would be better to present unsummed analysis and summed -
>that way we have the information we need to make a decision on what
>numbers are more appropriate.
>
>2. Alpha levels: In the meeting they mentioned they were running the
>analysis using an alpha level of 0.05. The alpha levels represent the
>probability of making incorrect conclusions that the treated sample is
>toxic or contains chemical residues not found in the control or
>reference sample (Type 1 error). By setting this probability low
>(0.05), the likelihood that one erroneously concludes there are no
>differences among the mean responses in the treatment, control or
>reference samples (Type 2 error) increases (low power). Type 2 errors
>would lead to conclusions that the sample is not toxic (or different
>from control or reference), when in fact there is a difference. Type 2
>errors are important to minimize in environmental investigations, since,
>if left undetected, these errors can lead to continued short- and
>long-term effects (ASTM 2003; EPA 2000a). In order to avoid this, an
>alpha of 0.1 can be used (and is in the work plan), which would increase

>the power of the test and the probability of detecting a reduction
>relative to the control mean. They are currently eliminating some
>samples on the basis that they are indeterminate in difference from the
>control at an alpha of 0.05 (I think from the meeting there were about
>11 eliminated). However, they may be determinate at an alpha of 0.1.
>These low responses may be important in the model - especially the FPM.
>In the work plan they state "if the analysis of the toxicity test data
>finds that the power for the data set is low, the alpha level may be
>raised to 0.1 as suggested in ASTM guidelines (2003)." From the meeting
>there was not mention they were moving forward with that analysis,
>however, I would recommend the report should include the analysis at an
>alpha of 0.1 and indicates how this changes the conclusions.
>
>3. What contaminants should be eliminated from the model: This relates
>to removing contaminants on the basis that they are not drivers of
>toxicity (e.g. aluminum). However, Mike A's analysis showed that some
>were slight predictors of toxicity. It may still be removed later on
>the basis that it is not a toxicity driver, but the report, (and their
>analysis) should include these contaminants (see "3" below). The
>analysis (at least for the FPM) will clearly show contaminants that
>aren't driving toxicity, and this will provide justification for
>dropping contaminants.
>
>4. The results of the bioassay tests and modeling effort may show that
>additional lines of evidence may be important in interpreting the
>bioassay results (e.g. EqP or pore water testing).
>
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>
>Larger Issues Include (may need more manager input):
>
>1. Running the FPM - there are still discrepancies between Teresa and
>Mike's models that must be resolved at a fundamental level. We don't
>want to be dealing with problems in replicating the FPM further down the
>line when we are also having to analyze results. I would recommend that
>these issues be worked out prior to submittal of the report, but more
>importantly that ALL steps she takes to get the FPM values be explicitly
>written out for each chemical / decision made. This should be at the
>detail that someone reading the report can replicate what was done.
>
>2. Discrepancies between the FPM and the logistic regression results:
>PAHs are a good example of this. The FPM method is calculating very
>high dry weight concentrations of PAH threshold numbers using this
>method that the government team does not agree with (and Jay has said is
>a non-starter).
>
>2. What endpoints should we be considering? The Hyalella growth
>endpoint appears to be producing different results than the other test
>endpoints. Teresa wants to remove this from her analysis because it is
>not producing reliable results, even though it is being used as a part
>of the logistic regression modeling. I don't think the team members
>agree with this assessment. I would recommend model runs for this
>endpoint should be included in the report, along with pooled endpoint
>runs that include this endpoint. We can then assess what it means after
>we see the data.
>
>3. What do we want the models to do? Loraine brought up this point and
>it is a very good one. Do we want the model to provide information on
>the chemicals detected in Portland Harbor or find the most predictive
>component that is predictive of toxicity (e.g. even if it is a
>conventional parameter)? You can run the models and get numbers for
>each chemical - if it is not contributing to toxicity this number will
>most likely be the AET from the dataset. However, I think this is
>useful information to anyone reviewing this report. I would recommend
>that most chemicals be run in order to justify their removal (which is
>easy running the FPM, but maybe not the logistic regression). Mike
>Anderson did this very quickly, and showed that some chemicals were not
>contributing to toxicity on the basis of the analysis. Numbers behaving
>in this manner were flagged with an AET value. By doing this it is easy
>to see that contaminant X wasn't a driver for toxicity at the highest
>detected concentration of X. This information is useful. The
>alternative is to find the most predictive indicator of toxicity, which
>may be a conventional parameter such as bulk ammonia, bulk sulfide or
>percent fines, or it may include a very limited list of contaminants.
>The downside here is that this approach may provide limited data on a
>wider list COPCs. If we go this route, bioassays to validate the model
>should definitely be done, and realize that it will not translate easily
>into cleanup numbers.
>
>4. What hit/no hit thresholds should we be considering? We gave some
>direction in our memo to them. However, they resisted going to the same
>thresholds between methods (for the FPM) in order to comply with
>consistency with other programs (which is odd because the "other
>programs" are still Teresa's work, but for Washington State). We had
>originally proposed using 10, 20 and 30 (or 90, 80 and 70) to correspond
>with NOAA's levels. Teresa did stat only, 10 and 25 for Washington
>State. Therefore, we got pushback on using the NOAA thresholds for
>Teresa's FPM analysis. Jay seems to think this is o.k. because the
>threshold levels don't matter too much as long as you get information at
>several levels for the model. I agree with him for the logistic
>regression model (because eventually you are developing a continuous
>model for which you can pick anywhere on the curve to correspond with
>magnitude of toxicity and prob of toxicity [jay correct me if I am
>wrong] for use in management objectives), but this is not the case for
>the FPM. Magnitude of toxicity (hit/no hit) levels need to be selected
>before hand and that is all the data you will have to make decisions.
>You can't for example select another threshold (e.g. something between
>the 10 and the 25) without re-running the analysis because you do not
>have a continuous distribution like the logistic regression model. We
>concluded that because of the resistance and since Mike had the data he

>could run the 10, 20, and 30 for the government team and we could
>analyze any differences between the different levels of magnitude of
>toxicity. However, it would have been better to stay consistent, and I
>think the three levels indicating magnitude of toxicity would have been
>helpful in interpreting the data for the FPM.
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>-Jennifer
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>-----Original Message-----
>From: Blischke.Eric@epamail.epa.gov
>[mailto:Blischke.Eric@epamail.epa.gov]
>Sent: Wednesday, December 07, 2005 12:13 PM
>To: pj.bridgen@envintl.com; jeremy_buck@fws.gov;
>Goulet.Joe@epamail.epa.gov; PETERSON Jenn L;
>Shephard.Burt@epamail.epa.gov; Robert.Neely@noaa.gov; OMEALY Mikell;
>chris.thompson@eiltd.net
>Subject: Fw: Summary of the November 21st Benthic Meeting
>
>
>Attached is the meeting summary that we discussed this morning.
>
>Eric
>----- Forwarded by Eric Blischke/R10/USEPA/US on 12/07/2005 12:12 PM
>-----
>
>   Lisa Saban
>   <lisas@windwarde
>   nv.com>   To
>   Joe Goulet/R10/USEPA/US@EPA, Eric
>   12/06/2005 05:15   Blischke/R10/USEPA/US@EPA
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>
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>   Subject
>   Summary of the November 21st
>   Benthic Meeting
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>Hi Joe, attached is our write-up of the summary of the meeting. Let me
>know if you want to add, edit, delete action items, etc. Also, let me
>know if you want to have a conference call on any of the issues (outside
>of the Teresa/Mike calls and the Lorraine/Jay calls). We are moving
>forward and are targeting a early Feb submittal date for the report.
>Thanks, Lisa
>
>Lisa Saban
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>
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>(206)577-1288, or by electronic mail, lisas@windwardenv.com.
>
>(See attached file: Summary of Nov21 Benthic Mgt.doc)
>

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